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ON

## ARTIFICIAL DILATATION

OF THE

## OS AND CERVIX UTERI

BY FLUID PRESSURE FROM ABOVE:

A REPLY TO DR. KEILLER OF EDINBURGH, AND ARNOTT AND BARNES OF LONDON.

BY

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[Reprinted from "The Boston Medical and Surgical Journal," for July 2, 1863.]

BOSTON:

DAVID CLAPP, PRINTER.....334 WASHINGTON STREET.

1863.



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AND MEDICO-CHIRURGICAL SOCIETIES OF EDINBURGH, ETC.

[Read before the Suffolk District Medical Society, May 30th, 1863.]

THOSE who are interested in obstetric surgery can hardly have failed to notice a controversy, for many months past carried on through the more important British journals, involving the question of priority as to suggestion and practical application in a matter of much importance—namely, the dilatation of the cervix uteri *from above*, as a means of diagnosis and treatment.

As one of the original claimants of the suggestion referred to, and, as I supposed, till within a few weeks, the only one with any legitimate ground for such claim, I have felt some little interest in the result. Not caring, however, again to enter the controversial arena, I should continue to remain a passive spectator, did not a more careful examination of the whole matter, to which I have been led by some recent allegations, compel me to break silence in simple justice to one of my opponents. It will be found, also, that this communication will not be without its value as bearing upon and instancing the law which should govern physicians, as all other scientific men, in the settlement of similar disputes.

Immediately on entering practice, it became evident to me that the great field for advance in obstetric therapeutics was *the interior* of the uterus—an opinion that was daily strengthened during the intimate relations to which I was admitted by Prof. Simpson in 1854-55.

At that time the sole means, at all safe and reliable, of directly reaching the interior of the unimpregnated uterus, was by the use of expansible tents, then only made of sponge, first suggested for this purpose by Simpson in 1844.\* It is true, that for the induction of

\* Edinburgh Monthly Journal of Medical Science, Aug. 1844, p. 734. Obstetric Works, Vol. i., p. 125, Scotch edition; p. 128, American do.

premature labor, for which the method to be considered was first proposed to the profession by both Dr. Keiller and myself, and for which its use is now urged by such competent authority abroad, there had been many measures suggested and practised—all of them, however, acting either secondarily or by reflex action, as do galvanism, mammary irritation, puncture of the membranes, their separation from the uterus by bougies, the injection of water or air, an agent here so dangerous, or by the uterine sound, and also, there seems to me good reason for believing, the so-called oxytoxics, as ergot of rye; or by a stimulating or dilating force applied and first acting from below.

These remarks apply with equal force to all methods that had then been proposed—to those of Hamilton and Hopkins; to the flexible catheter left in the cavity of the uterus by Merrem, Krause and Simpson; to the vaginal and cervical plugs and dilators of Brünninghausen, Osiander, Von Busch, Hüter, Gariel and Braun; to the carbonic-acid douche, suggested by Brown-Séquard and so fatal in the hands of Scanzioni and others; and to the water douches, of Kiwisch, applied to the vagina, and of Schweighäuser and Cohen, to the uterine cavity. These several means, while they were applicable but partially and with varying success to the pregnant uterus, were wholly unfitted, with the exception of sponge tents, for opening up that which already contained no foetus; and for this the elastic bougies of McIntosh and the unyielding ones of Simpson, the spring-knife of the latter, the hollow tubes used by Wakley for urethral stricture and adopted from him by Baker Brown, and the instruments of Rigby, Graham Weir, Osiander, Busch, Krause and Jobert, with expanding metallic blades, are either insufficient or attended with too much hazard.

Caoutchouc bags or sacs, distended with air, had been proposed some years previously by Gariel,\* for the treatment of displacements of the uterus by pressure from below, and for plugging the vagina in cases of haemorrhage. He had also suggested their possible introduction into the cervix, not, however, through it, for the purpose of overcoming stricture of that canal, and had even asked, "if this property of the bulbous air sound could not be turned to advantageous use in inducing premature labor?" here referring, however, to their use in the vagina, as had already been suggested by Hüter and Braun. The proposals of Gariel, however, like those of Braun, were attended with singularly unfortunate results, Breit and others reporting a mortality of six patients out of fourteen.†

To sponge tents applied to the cervix there attach, as I have already intimated, various important objections. They are readily acted upon chemically by the uterine and vaginal secretions, and from their organic character quickly undergo putrefactive decompo-

\* Gazette des Hôpitaux, 1849, No. 141.

† Göschen's Deutsche Klinik, Berlin, 1853.

sition, subjecting the patient to a certain amount of risk from such possible absorption as is hereby implied. They act at times with great rapidity and force, and where the tissues are morbidly friable, if not very carefully made, they may produce unintentional or dangerous tension and laceration.

From direct experience of these several dangers, it became my aim to find, if possible, a substitute for sponge in the dilatation of the cervix, and in May, 1855, in a paper read before the Medico-Chirurgical Society of Edinburgh, I proposed the use of tents prepared from the bark of our indigenous slippery elm.\* Shortly after, during the publication of Dr. Simpson's Memoirs, I had again occasion to refer to the disadvantage of sponge under certain circumstances,† and at still greater length in a paper presented during the fall of the same year.‡

The use of elm tents in my own hands and those of others who have communicated with me upon the subject, proved the agent greatly superior to sponge in those cases where a slow and moderate action is desired, as, for instance, in mechanical dysmenorrhœa and certain forms of sterility; and as yet I know of nothing that will here better answer the indication, although during the course of my experiments in this direction I have tried a variety of other substances, as althea root, &c., among them the root of gentian, afterwards made the basis of a memoir upon mucilaginous tents by an English Surgeon, Dr. Aveling, of Sheffield,§ in apparent ignorance of his having been anticipated by my suggestion of three years before.

Steadily pursuing these efforts towards the solution of the interesting problem proposed, I again called the attention of the profession to its importance by a paper published in Philadelphia early in 1859, in which were pointed out the several indications for artificial dilatation of the cervix uteri, and the several dangers attaching thereto, alike in the induction of premature labor, the assistance of the progress of accidental abortion and of labor at the full time, the exposure of the uterine cavity for the purposes of diagnosis and treatment, both in diseases puerperal and non-puerperal.||

During the preparation of this paper, duly appreciating, as will be apparent from its perusal, the actual and relative value in the assistance or induction of labor, of the several elements of action involved—namely, dilatation of the cervical canal, detachment of the membranes from the walls of the uterus, and the prolonged preservation intact of the bag of waters—I had frequent conversations upon the subject with my friend Dr. Nathan Hayward,

\* Association Med. Journal, London, May, 1855, p. 446; Glasgow Med. Journal, April, 1856, p. 116; Braithwaite's Retrospect, Jan., 1857, p. 247, &c.

† Preface to Simpson's Obstetric Works, Sept., 1855, p. 16.

‡ This Journal, Nov., 1855; Gardner, Causes and Treatment of Sterility, 1856, p. 148.

§ Medical Times and Gazette, June, 1858, p. 653.

|| "The Use and Abuse of Uterine Tents." American Journal of the Med. Sciences, Jan., 1859, p. 57.

of Roxbury, now Surgeon of the 20th Mass. Regiment, and at that time associated with me in the conduct of the Eustis St. Dispensary. With his assistance, I contrived an instrument designed to combine the various indications just referred to, and this was used in practice upon the first favorable case that presented itself to us, on April 13th, 1859. The operation was entirely successful; labor was prematurely induced at the eighth month in a woman who had four times previously undergone craniotomy, and a living male child was delivered. The case was the more interesting to us from the fact that both Dr. Hayward and myself were present at her last confinement; I had turned and delivered the trunk, but it was found absolutely necessary to lessen the head from below before it could be made to pass.

The proposal of the measure now resorted to, as I supposed, for the first time, was made at considerable length under the name of "the uterine dilator," and the case reported in July, 1859.\* I then stated that the instrument, introduced within the cavity of the uterus, produced its action in a threefold manner: "reflexively, as a foreign body; reflexively and directly, by separating the membranes from the uterine walls; and directly, as a fluid wedge, by dilating the os; in each of these three respects, its effect being in proportion to the amount of distension applied. It should be noticed that this dilatation," I also added, "is *from above downwards*, while the tent dilates from below upwards."<sup>†</sup> I referred to the similarity of this instrument to one suggested for the female urethra by Spencer Wells, of London, some months previously,<sup>‡</sup> which in its turn had been taken from a modification by Thompson of James Arnott's urethral dilator, so forcibly brought forward as long back as 1818, both by himself<sup>§</sup> and his brother Neil,<sup>||</sup> and shortly after by Ducamp in a memoir that received much approbation from the French Academy. I mentioned, also, the curious fact in the history of the various means that have been proposed for dilatation of the uterus, that they have all, without exception, been based, directly or indirectly, upon some method previously in use for the treatment of strictures of the male urethra.

I have thus plainly stated my own position in relation to the plan of dilating the uterus by fluid pressure acting from above, and have shown the gradual and successive steps by which I arrived at the idea and its development. The medium employed for dilating my sac was water; to the dangers of air used for this purpose, as it has been by others who have taken part in this controversy, I then called at-

\* Am. Jour. of the Med. Sciences, July, 1859, p. 107; North Am. Med.-Chir. Review, July, 1859; Essay on Criminal Abortion in America, Phila., 1860, p. 69.

<sup>†</sup> Loc. Cit., p. 112.

<sup>‡</sup> Medical Times and Gazette, July, 1858, p. 84.

<sup>§</sup> Treatise on Urethral Stricture, &c. &c.

<sup>||</sup> Elements of Physics, p. 532.

tention, as I shall again do in the course of the present communication.

Now as to opposing claims, which I shall endeavor to state as fairly, even at my own expense.

In March, 1859, some six weeks earlier than the date upon which my own patient was confined, Dr. Alexander Keiller, of Edinburgh, a gentleman of great obstetric knowledge and skill, to whose ingenuity in another matter, the suggestion and application of the vaginal stethoscope, I chanced to call attention in the very paper containing the description of my own dilator—having independently conceived of the same idea, put it into successful practice in the presence and with the assistance of our mutual friend, Dr. Graham Weir. The case was immediately reported to the Obstetrical Society of Edinburgh, and was mentioned by Dr. Keiller in conversation and at his lectures, but strangely enough none of the details were put in print until the publication of a summary of the Society's Records, on the very day of my own paper, namely, the 1st of July, 1859.\* This was a brief abstract of Dr. Keiller's remarks, by the Secretary; his own first publication upon the subject, with the exception of three short and bitter controversial notes,† in the latter of which he did indeed quote from the Proceedings of the Obstetrical Society already referred to, was not till a period of four years afterwards,‡ although the profession had on more than one occasion been promised an immediate communication.

On April 16th, 1859, just three days after my own application of dilatation by fluid pressure from above to actual practice, Mr. Jardine Murray, of Brighton, England, a former Resident Surgeon at both the Royal Infirmary and Maternity Hospital of Edinburgh, introduced an India-rubber air pessary, in a case of haemorrhage from placenta praevia, into the cavity of the uterus, with the double intention of thus controlling the haemorrhage and of effecting dilatation of the os. Mr. Murray very honorably acknowledged his obligation for the idea to his former instructor, Dr. Keiller, by whom, however, he was bitterly assailed in the letters above referred to; and his case is undoubtedly entitled to its claim of being the first of the kind put on record by publication, and the first in which the dilator seems to have been used as an intra-uterine plug for arresting haemorrhage. This publication was in June,§ a fortnight before either my own views or those of Dr. Keiller appeared in print.

In April, 1861, Dr. Robert Barnes, of London, well known for his contributions to obstetric literature, brought the subject we are considering before the Obstetrical Society of London,|| his first ap-

\* Edinburgh Med. Journal, July, 1859, p. 84.

† Med. Times and Gazette, June 18th, 1859, p. 639; Ibid., July, 1859, pp. 24, 75.

‡ Read before the Obstetrical Society of Edinburgh, Aug. 6th, 1862; abstract from the Society's Records printed in Edinburgh Med. Journal, February, 1863, p. 763, and the paper itself in the same Journal, March, 1863, p. 782.

§ Med. Times and Gazette, June 11th, 1859, p. 506.

|| Transactions of the Obstetrical Society of London, Vol. iii., 1862, p. 107.

plication of the method to practice having been with a case of *placenta prævia*, in April, 1860, just a year subsequently to those of Dr. Keiller and myself. His paper was received with marked attention, and the discussion which followed is well worthy of general perusal. Dr. Barnes discarded, as I had already insisted should be done, the use of air for purposes of uterine dilatation, and claimed, as I had done, that water was the only allowable medium—going on to assert that by this means “the practitioner was enabled to deliver almost at will, not only on a fixed day, but at a predetermined hour; a power that gives us control over cases of convulsions, obstinate vomiting, exhaustion from disease or haemorrhage, much needed and not hitherto possessed.”\*

In a subsequent paper, a year later, upon “the new method of inducing premature labor at a predetermined hour,” Dr. Barnes seems more decidedly to claim the proposal as originally his own,† whereas, in fact, he but modified the shape of the dilating sac, making it “fiddle-shaped,” so as to act both from above and below, a nicety that in practical application possesses little or no advantage over the original form.

Finally, during the last month,‡ there appeared a communication from Dr. James Arnott, of London—to whose celebrity in former years for his various applications of fluid pressure and congelation to medical and surgical practice, I have already alluded—calling our attention, by name, to what he considers forgetfulness or intentional omission. The article to which I now refer is nearly identical with a letter by the same gentleman,§ shortly after our first proposals in 1859.

It will have been noticed that in my own first communication, I acknowledged the fact that the instrument then proposed, like that suggested for the female urethra by Spencer Wells, was really based upon Arnott’s dilator for the male.|| Dr. Keiller, in his paper of March last, allows that his own conception of the idea was from the instrument of Mr. Wells,¶ and Dr. Barnes also acknowledges that the original suggestion of fluid pressure for purposes of dilatation was by Arnott.\*\* So far as I can ascertain, however, though Arnott in the various publications to which he has lately referred did recognize the real action of the fluid wedge, by which the distended membranes produce dilatation in labor, his application of the theory was to the os *from below*, or in the course of the cervix, if this canal

\* Medical Times and Gazette, April, 1861, p. 456.

† Ed. Med. Journal, July, 1862. It is to this paper that the profession are probably indebted for the actual publication of Dr. Keiller’s views upon the subject, some nine months later, they being professedly in answer to Dr. Barnes’ claim.

‡ Ibid., April, 1863, p. 968.

§ Med. Times and Gazette, July, 1859, p. 69.

|| Am. Journal of the Medical Sciences, July, 1859, p. 108.

¶ Ed. Medical Journal, March, 1863, p. 784.

\*\* Trans. of Obst. Soc. of London, Vol. iii., 1862, p. 120.

remained uneffaced, and not *from above it*; an operation of entirely different nature, based upon an entirely different principle, and no more to be claimed by Arnott than those of Braun and Brünninghausen.

Having thus stated all the facts in the case, it will be seen that, putting aside the measures of Hüter and Braun for inducing premature labor by dilatation of the vagina as entirely foreign to the subject, it is to Arnott and Gariel that belongs the credit of first suggesting the possibility of dilating the cervix uteri by fluid pressure directly applied to that canal, in these instances from below; that in dilating the uterus by fluid pressure from above, although my instrument was already prepared for the purpose, Keiller really anticipated me by a few days in actual practice; that we both immediately made our discovery known to medical friends and thus to the profession, and that our respective publications in print were made on the same day, in Philadelphia and Edinburgh. As regards priority of publication, however, Mr. Murray certainly forestalled us both, his case being the first thus recorded. Personally, I do not hesitate thus far to yield the credit to Dr. Keiller, merely claiming for myself independent conception and suggestion. To Dr. Barnes belongs the merit of forcibly presenting the subject to the profession at a later date, of endeavoring by modification of our instruments to perfect one for practice, and of adopting my proposal of water as the dilating medium.

So far, as regards our several claims to the original proposal of dilatation of the cervix by pressure from above. I have referred to the value of water as compared with air for the dilating medium. This to my mind is practically as important as the idea of the dilatation itself, for it is a question that may often directly involve the life of the patient. Cases are already on record of sudden death from admission of air into the cavity of the uterus, especially towards the close of pregnancy and during labor. I need only refer to those instanced by John Reid,\* Simpson,† May,† Barry,§ Depaul, Gardner, Dalton and others; and whether we are to suppose the fatal result produced by the passage of air into the abdominal cavity through an abnormally patent Fallopian tube, or its forcible injection thither by the uterine contractions, or are to accept the more probable alternative, as suggested by the younger Legallois in 1829, by Ollivier in 1833, and more recently by Reid, Simpson, Cormack,|| and McClinton,¶ that the air is forced directly into the circulation through the uterine sinuses, and so kills by inducing paralysis of the heart from overdistension, or asphyxia from more gradually increas-

\* *Physiological Researches*, 1848, p. 578.

† *Obstetric Works*, i., p. 719, ii., p. 74; *Edinburgh Medical Journal*, Sept., 1861, p. 289.

† *British Medical Journal*, June, 1857.

§ *Prov. Medical and Surgical Journal*, Nov., 1850.

|| *London Journal of Medicine*, Vol. ii., p. 950.

¶ *Medical Press*, March, 1852, p. 147.

ing obstruction of the lungs, it is impossible to lose sight of the danger. To this I have repeatedly called attention on former occasions, and a marked and fatal instance of its effect has just been communicated to me by my friend Dr. Hitchcock, of Fitchburg. In view, therefore, of the risk referred to, I have not hesitated to impress upon the students at present temporarily in my charge, the extreme caution necessary in manual or operative interference during labor, the impropriety of endeavoring to excite intra-uterine or intra-vaginal respiration, even by the method so ingeniously suggested by Dr. Jacob Bigelow, of this city,\* and also the possibility of air sufficient to produce fatal syncope being thrown into the uterine cavity where premature labor is induced by the injection of water between the membranes and uterine walls, as in cases of death related by Guillier, Germann, Chiari and others.

In the instances of the new method reported by Drs. Keiller and Murray, dilatation was effected by India-rubber sacs into which air was forcibly thrown. Against rubber for uterine or vaginal application, used in any form except vulcanite, which as yet is afforded us in too unyielding a state, there is the insuperable objection that it is chemically acted upon by the fluids with which it comes in contact and becomes at once offensive and irritating. If distended to any great extent, it is very liable to rupture, and if this be guarded against by an increase of thickness, the introduction of the sac becomes proportionately more difficult. In the use of thin animal membrane for dilatation the same liability to rupture exists, unless, as suggested by Arnott, a delicate layer of a stronger substance, as silk, be interposed between two layers of the membrane. In practice I have more than once ruptured the membrane during its distension by water, and therefore know that what I have asserted of such danger where air is used, is not unfounded. What, then, can be said of the deliberate use and recommendation of air under these circumstances? Is it not an unjustifiable exposure of the patient's life to a grave and unnecessary risk?†

To one other objection that might be alleged I must call attention, merely to state my belief that it is unfounded. There is no doubt that in the induction of premature labor by the injection of water at random between the membranes and the uterine wall, after the method of Schweighauser and Cohen, and as is now so commonly done, there is some liability of effecting an unfavorable change in the presentation of the child, and of inducing haemorrhage by partial separation of the placenta. It is also possible, as in two cases lately related by Simpson,‡ that rupture of the uterus, from excessive overdistension, may thus be produced. These remarks, however, do

\* American Journal of the Medical Sciences, April, 1829, p. 285.

† I might refer to various other points, of practical importance in this connection, but prefer to leave them in the hands of an intelligent and talented gentleman of the present medical class, Mr. Greene, of Fitchburg, who is preparing a monograph upon the subject.

‡ Edinburgh Medical Journal, Sept., 1861, p. 290.

not legitimately apply to the subject now under our consideration. The extent of separation of the membranes, of dilatation of the cervix and of additional distension of the uterus, by the use of the enclosing sac we have proposed can be kept perfectly under control. The amount of dilatation and its exact location are accurately known from the size and position of the sac, and, in case of necessity, by a mere turn of the stop-cock we can at once effect the entire escape of the fluid and collapse of the dilatorium.

The method we have now discussed, it will have become evident, is applicable not merely to cases requiring the induction of premature labor, but wherever for any other reason it is desirable to have free access to the uterine cavity, as for the removal of tumors, &c. &c. A consideration of these circumstances, however, I shall reserve for another occasion.

